Errata Sheet

COMMENTS ON
THE FRIENDS OF THE SEA OTTER CRITIQUE OF
THE STATE OF CALIFORNIA SEA OTTER PROPOSAL

On page 1, line 11, delete "that we not ask for a waiver of the moratorium to take sea otters;"

On page 24, line 2, change "not" to "now."

On page 40, delete lines 2-6, and add:

> The Department of Fish and Game was advised by the U. S. Fish and Wildlife Service that our proposed management and research plan for sea otters could be approved and jurisdiction returned to the State without a waiver of the moratorium. The Department subsequently on June 24, 1976 asked the Director of the Fish and Wildlife Service to withdraw our request for the waiver of the moratorium.

On page 42, line 6, change "action of" to "action or."

COMMENTS ON

THE FRIENDS OF THE SEA OTTER* CRITIQUE OF

THE STATE OF CALIFORNIA SEA OTTER PROPOSAL

by

California Department of Fish and Game

Sacramento, April 1976

INTRODUCTION

Most of the comments presented in the FOS critique were based on differences in values and policy rather than errors in the proposal. In most cases, the FOS comments were not accompanied with factual documentation, and a lack of understanding appears to exist on the part of the reviewers of not only the dynamics of the sea otter but of resource management techniques and status of the shellfish resources.

Since only one factual error was found (a mathematical computation in Appendix F) and the other suggestions for change were not agreed to or of sufficient magnitude to warrant complete retyping of the proposal, the decision was made to submit the proposal to the FWS with minor corrections. Some of the unacceptable changes were: that we not ask for a waiver of the moratorium to take sea otters; that the program be limited to five years; that the sea otter be considered endangered or threatened; and that the simulated population curve "should not be permitted to enter the final draft". We also consider Roest's synonymy valid until new information is available.

This DF&G response to the FOS critique is addressed only to the more important issues in question, especially in those few cases in which specific examples were presented by the FOS to relate *"errors"* in the proposal.

*In these comments, FOS = Friends of the Sea Otter; FWS = Fish and Wildlife Service; DF&G = California Department of Fish and Game.

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RESPONSE TO FOS CRITIQUE

Paragraph 2. *"the document is very long, repetitive, and diffusely organized"*

It must be remembered that this proposal is not a concise scientific paper on findings of a research program. It has been written in the form of an EIS in which references are kept at a minimum and wherein much of the content is in a narrative of the different impacts the proposal would create. However, this proposal does include a scientific summary of known facts and theory on a vast number of species and fisheries, and reference material was included along with some raw data. This resulted in a long and at times purposefully repetitive document. It should be mentioned here that this proposal will be rewritten jointly by the DF&G and the FWS, and shortening and possible reorganization of the document is expected. The purpose of including all the available information and repeating some series of data in several places was to make it easier for the readers to understand certain data that applied to several aspects of the proposal without excessive cross-references throughout the document.

Our first proposal submitted in 1974 was returned by the FWS to the State requesting more detail on why certain recommendations were made. To satisfy the FWS format to include substantiating data and speculative conditions of poorly known aspects of the problem, it was not possible to present this complex sea otter conflict in any concise manner. Thus, the task was attempted to provide a detailed explanation of one of the most complex resource conflicts to ever occur in California. This is why so much basic information, facts as well as speculation and theory, have been presented. Considering some apparent lack of understanding of marine resource management procedures and techniques and of the sea otter's life history expressed in the FOS critique, even more detail should have been given to relate some of the more intricate relationships between man, sea otters, and shellfish.

Paragraph 2 (cont'd). *"its information presented with a goal-directed bias rather than in an impartial, scientific way..."*

The sea otter policy of the State of California since 1913 has been one of complete protection and unlimited movement. This policy and management procedure continues, but now the State wishes to change its policy to one of experimental containment. The impact to the animal and to the affected fisheries by a non-containment management regime is described in this proposal as well as for containment management. In making this proposed change in management, we have indeed *"assessed the status of the sea otter and improved it..."* Our proposal is not entirely directed to containment or *"permanent curtailment of the otter's range"*. We have prepared documentation for both these contingencies in case the decision is made to undertake either management scheme. The purpose of this proposal is to supply information and suggest research that can lead to a variety of possible management regimes, one of which is potential permanent containment. The impacts of protection in either containment or non-containment managements to the animals have been presented in conjunction with the impact of the otter, both predictable and theoretical, on the ecosystems that it may occupy. Examples from the proposal:

Pages 9-10:

> "A principal cause of mortality in harbor areas is being run over by boats, and even though this mortality is relatively small and does not materially affect population levels, it presents a serious management problem. The safety and protection of individuals as well as subpopulations of a species is a costly element of management and must be given high priority. There is also concern about the increased assimilation of chemical pollutants if the sea otter is allowed to immigrate into areas of intense human activity; and potential threat to animals from catastrophic as well as from occasional small oil spills is of utmost consideration."

Page 10:

> "This request for a waiver of the moratorium to take sea otters is designed to meet the protection needs of the sea otter and to conduct further research on sea otter population dynamics, behavior, habitat requirements, pathology, and biology."

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Page 11:

> "Because of state responsibilities and requirements of the Act, this request for the take of sea otters and research is designed to contain a non-threatened and healthy sea otter population within a segment of the California coastline until the secondary effects of sea otter foraging is clearly documented."

Page 153:

> "The extension of the population north to Miramontes Point and south to Point San Luis (Avila Harbor area) is to ensure that all sea otters in California cannot be threatened by a major oil spill."

Pages 153-154:

> "It is axiomatic that the translocation of sea otters from the southern range limit cannot continue once the carrying capacity of the total range from Avila to Miramontes Point is reached. When this happens, the initial purpose of establishing a non-threatened expanded population will be satisfied, and it would create serious husbandry problems for the sea otter to continue translocating animals to the north of Santa Cruz. Alternate management proposals must be initiated at that time; and in the interim period intensive research on the effects of removal of extralimital animals at the southern periphery will be conducted. Research on the secondary effects of sea otter foraging will be completed by that time, and many of the partly understood parameters of population dynamics will be disclosed in research studies. Baseline studies to be initiated near Avila at the earliest possible date through special research funding will blend into increased research on the effects of sea otter foraging near Santa Cruz. In the interim, intensive studies would be conducted on the dynamics of animals near the peripheral range limits to determine the age and sex of the animals leaving the range and from what areas within the range these animals are emigrating.
>
> "When all these data are analyzed, new proposals will be submitted to manage the sea otter when the proposed range from Avila to Miramontes Point is occupied by an established population of sea otters. It is not possible at this time to understand the sum total of the effects of foraging and the dynamics of the results of removal of extralimital animals from the south, ..."

In the statement on pages 153-154, permanent containment was not suggested as a primary management scheme. In fact, the first mentioned possible management plan was to continue the present management of non-containment. The impact of non-containment appears on pages 245-246:

> "a. <u>Biological Consequences</u>:
>
> "Eventually, the entire coastline and offshore islands will be occupied by sea otters. Projected standing crop estimates of the aboriginal

> population indicate a maximum population of about 16,000 animals could inhabit California's shoreline (page 100). This unharvested and uncontrolled population stock would surpass optimum and maximum levels resulting in die-offs of animals above the carrying capacity, followed by population buildups and subsequent repeated die-offs. The principal age group that is affected by starvation mortality are pups, and a constant public relations problem would exist throughout the state on the handling of beached moribund pups that will be brought to the attention of management authorities. Full protection of a very adaptable animal throughout the state would require considerable increase of enforcement. In a statewide population of 16,000 animals with a potential maximum reproductive rate of 15 percent, at least 2,000 animals a year on the average must die to match annual reproduction in a saturated maximum level population. Since population crashes are usually seasonal and do not occur at the same rate in each year, there could be exceptionally harsh winters or massive changes in kelp density due to warm water in which there could be a massive die-off involving many more than 2,000 animals. The injury to animals in the heavily congested harbor areas of southern California and San Francisco Bay will cause considerable trauma for animals that would constantly be venturing into these areas."

We agree that there is a *"slant toward a single view of management."* There is a descriptive emphasis on containment management inasmuch as this document is the first printed collation of existing data proposing such a management regime. We have observed the otter for many years under the management of non-containment, and the results of this procedure are well documented and predictable, both in the basic behavior of the animal and the effects of foraging on certain shellfish fisheries. There is little need to elaborate on this procedure other than what has already been stated in the proposal and documented in the literature. In the Preface of the proposal, this subject is summarized pointing out that we have not made a foregone conclusion as to what management regime will be suggested:

> "When the entire proposed range from Miramontes Point to Avila is occupied by sea otters, evaluation of population dynamics studies and secondary effects foraging will determine future management procedures."

All the baseline studies, research on sea otter population dynamics and behavior, and concern about oil spills and other pollutants are applied as much to non-containment as they are to containment management procedures.

Paragraph 3.

Phrases occur in this paragraph that reveal a lack of understanding on the reviewers' part of not only scientific methodology but of the purpose and format of this proposal that was prepared in the form of an EIS. Some of these phrases are: *"misleading use of facts", "mixing of facts and estimates", "upon mixtures of fact and theory",* and *"various factual errors."* It should not be considered unscientific to include the essential elements of scientific investigation and reporting; e.g., facts, estimates, and theory. These elements are present in scientific publications and are all essential to describe scientific works. Estimates of population parameters are considered facts (accompanied with the necessary assumptions), and theory is often submitted to show where facts and "marginal" data may lead and how to test deductions and cause-and-effect relationships. The *"factual errors"* mentioned in the FOS critique were in reality not errors and will be detailed in this response.

Paragraph 4.

The words and phrases *"incorrect", "misleading", "extend beyond present knowledge",* and *"between the lines"* cannot be responded to since they don't refer to particular statements in the proposal.

Paragraph 5.

I used the less common term "dominant competitor" rather than the more accepted "competitive dominants" because the former sounds more descriptive. I have put this term in quotations rather than change the text of the proposal.

The comment that *"the presentation does not give such specifics when using the term; it does not say which species the term applies to, on what basis it is used, or who is competing with who for what"* is inaccurate. All these supposed omissions were clearly listed in several sections of the proposal.

Pages 155-156:

> "The most dramatic change in benthic communities due to sea otter foraging is the reduction of 'patch diversity' of macro-invertebrates and plants. Exposed patches that are dominated by a single edible species such as sea urchins, abalones, mussels, or barnacles are removed by sea otters. These 'dominant competitor' communities contribute a unique richness and diversity to both the intertidal and subtidal zones. In the intertidal zone algal growth dominates both within and outside the sea otter's range. The more common species are *Fucus, Ulva, Endocladia, Iridaea, Egregia, Gigartina, Pelvetia,* and *Porphyra. Phyllospadix* and coralline algae also form dense clusters and patches in the intertidal zone. In calm areas in which sea otters can forage at high tide, exposed patches and individuals of sea urchins, mussels, and black abalones are greatly reduced leaving only interstitial populations of these macro-invertebrates. In rugged exposed intertidal areas where sea otters cannot safely forage at high tide, some upper level exposed patches of mussels and black abalones can persist. Sea otter foraged intertidal zones are highly dominated by algal growth."

The four primary groups (page 155 of proposal) are sea urchins, abalones, barnacles, and mussels. Also, several algal species that can form dense "patches" or can "dominate" substrate are listed by genus.

Page 158:

> "Dense patches of coralline algae are common both intertidally and subtidally in Carmel Bay within the sea otter's range, and in intertidal areas throughout the area from Monterey to Cayucos rich algae growth is typical."

Black abalones were mentioned on page 158, and data from Lowry and Pearse (1973) include urchin and abalone interactions.

Pages 158-159:

> "Lowry and Pearse (1973) pointed out the dense populations of abalones and urchins in these deep crevices subtidally and noted that even though exposed macro-invertebrates were rare, these crevice populations yielded a mean density of about 0.2 sea urchins and abalone per square meter in belt transects run through the area. Within these crevices, there is competition for space and drift food by urchins and abalones and possibly from starfishes and crabs as well, but studies are needed to better understand the interactions of these species. Lowry and Pearse (1973) explain the problem:
>
> > 'Since sea urchins and abalones occupy the same habitat and utilize the same primary food source, they probably influence one another to some degree. Cox (1962), Leighton (1966b), and Shepherd (1973) have suggested that, under certain conditions, sea urchins somehow

> "out-compete" abalones, particularly in competition for food. Where food does not appear to be limiting, such as in the Hopkins Marine Station kelp bed, abalones may directly or indirectly out-compete sea urchins for space. Such competition may account for the absence of sea urchins in the larger crevices at Hopkins Marine Station; the sea urchins may be (1) pushed out of the larger crevices by the actions of abalones, or (2) extracted from the larger crevices by sea otters more readily than are abalones. These alternate hypotheses are not exclusive, and they can and should be experimentally tested by manipulations of sea urchin and abalone populations within crevices.'"

Species and groups of dominant herbivores and algal forms are listed throughout the discussion of impact upon the community structure from pages 155 through 170. Not only were there repeated references to the species of competitors and dominant forms but in Appendices N and O are photographs and drawings of substrate covered with patches of urchins, mussels, abalones, barnacles, surf grass, etc.

Paragraph 7. *"The presentation should stop with the brief statement on page 71, rather than repeatedly trying to show that sea otters have no effect on stability of nearshore ecosystems"*

Nowhere in the proposal is there a statement that even hints that sea otters *"have no effect on stability"*. The entire discussion on pages 71 through 74 describes the complexity of the term stability and presents quotes of theoretical ecologists. Throughout this introductory discussion, the words "sea otter" appear only once and not in any statement saying they do not influence stability:

> "Most of the common algae in California as well as the principal macroinvertebrates utilized by sea otters are found throughout the state except that *Nereocystis* is not found south of Point Arguello, and *Macrocystis* canopies are sparse north of Half Moon Bay."

In section IV B 2 (pages 157-196), possible relationships between otters and algal growth are related in as much detail as appears in the literature. Again, throughout this section, there is no statement that infers sea otters do not influence stability. In fact, there is a quote from Estes and Palmisano that does imply there is a relationship with stability.

Pages 167-168:

> "'We believe that the sea otter is an evolutionary component essential to the integrity and stability of the ecosystem' is in theory a valid concept that cannot be applied to all marine ecosystems in North America, especially in terms of 'integrity' and 'stability' because these concepts are not empirically defined or available (page 71). Obviously, species' interactions in the Aleutians are radically different than those in the complex ecosystems of southern California where several keystone species may be interacting at the same time and place."

It is impossible for anyone to make definitive statements on the stability of ecosystems inasmuch as this concept is far from being empirically documented for subtidal communities. From the FOS critique, we read: *"To date, this issue is far from clear (period!), as is the relationship between stability and species diversity."* I heartily agree!

What is most perplexing in the FOS critique is the request that the proposal's discussion on stability be terminated on page 71, thereby omitting important quotes from two of the foremost researchers in the field. These ecologists have attempted to clarify the concept of stability and put it in its proper perspective. The State cannot keep such important clarifying information from lay persons and from the legal interpreters of the Mammal Protection Act of 1972 which states that the "primary objective of their management should be to maintain the health and stability of the marine ecosystem". The FOS critique agrees that health and stability cannot be defined, but it appears contradictory to suggest that the quotes from Paine and Ricklefs (page 72) should be omitted from the proposal.

Paragraph 8. *"The presentation would have more strength if it focused more on how much enhancement of primary productivity <u>might</u> result from sea otter's foraging activities as their range expands, how this <u>might</u> be determined... and the <u>possible</u> important consequences of this enhancement..."* (underlines mine)

It would have been proper in the FOS critique to have cited publications documenting such occurrences, but to my knowledge, such references don't exist. The only California information on this concept has been referred to in the proposal.

Pages 168-169:

> "The enhancement of understory algae due to sea otter foraging on herbivores is recognized, and primary productivity is thus enhanced in these areas or wherever drift kelp happens to be carried. Some of this increased algal growth will be eaten by herbivores, but in many areas such as that described above by Rosenthal, Clarke, and Dayton (1974) the present algal growth supplies ample food for large populations of invertebrates, most of which would be removed by sea otters leaving even more kelp to decompose. Note the dense patches of sea urchins and black abalones in the intertidal area of San Nicolas Island (Appendix N, Plates I and IV) that obviously feed on drift algae and have developed 'stable' communities considering the age of these large specimens. Unless there is a large increase in herbivore biomass after the removal of invertebrates by sea otters, the increased productivity in the form of algae will most likely not be utilized by herbivores, certainly not more abalones, sea urchins, and *Cancer* crabs that are reduced materially by sea otters. Alfred Ebling (University of Californi Santa Barbara, pers. comm.) reports that the large masses of drift *Macrocystis* that settle into low areas of reefs of the Channel Islands are too massive to be eaten by herbivores and deteriorate slowly by decomposition creating water conditions that fish species avoid. Possibly bacterial decomposers will increase in a situation such as this and eventually supply energy to more organisms in higher trophic levels, but at this time this is mere conjecture. The reduction of Pismo clams and gaper clams from sandy areas by sea otters does not increase primary productivity, and transect digs indicate no other large invertebrate species are occupying these areas."

These data are speculative, but they were included to indicate potentialities.

Paragraph 9.

The important matter here is the concept of the "optimal sustainable level" and the "carrying capacity" of the habitat. My response to the long sentence in the FOS critique which includes five *"facts"* on why the State's proposal *"gives very little supporting evidence for this claim"* will be given in sections, answering each one of the *"facts"*.

a. *"...the density of sea otters within their established range in California is well below that of Alaskan populations..."*

On page 100 of the proposal, a conservative estimate of about 12 animals per square mile of habitat out to 120 feet was given for the California population between Seaside and Morro Bay. This average figure includes all the bottom area whether it be rock, sand, or mud and irregardless of whether there is

kelp growing on the rocky reefs. Sea otter densities within California are not the same throughout the range; they vary from less than one animal per square mile along sandy beaches to large concentrations at the migrant fronts of up to 150 animals. These densities must be used with caution in that a concentrated large group during the resting period may forage over a much larger area, and the foraging distribution would represent a true carrying capacity density. Therefore, only larger geographic areas should be used to relate relative densities, such as 5- or 10-mile segments of the coastline. Note in Appendix F, the contiguous distribution of animals, mostly in aggregates of less than 4, throughout the established range with an occasional clustering of animals in larger rafts. Kenyon reported densities at varying localities of from 10 to 20 animals per square mile, and Estes and Smith reported around 60 animals per square mile at Amchitka.

The comparison as related in the FOS critique between Alaskan and California sea otter densities is not valid. There is a marked difference between the community structures in California and Alaska as well as of densities and species of food items, and the bottom topography is dissimilar. There are many sand channels and sand pockets between and on rocky reefs in California, whereas in Alaska most of the bottom is of rock without extensive sand areas and is, therefore, potentially more productive for rock dwelling sea otter food items. Other major differences in food supply include the presence of species of fish that can be captured by otter in Alaska (page 85 of proposal) that are not present in California (up to 40 percent of the Amchitka energy source to otters is fish); and there are more invertebrate species available to the otter in Alaska (Appendix L). In spite of the fact that off Monterey there are no fish species to be eaten and that at least 60 percent of the bottom out to 120-feet depth is sandy areas poor in food items, the densities along the Monterey Peninsula, per equal

area of rocky bottom, probably exceed those recorded by Estes and Smith at Amchitka. The reasons for this are that the rocky bottom on the Monterey Peninsula is granodiorite, containing many deep crevices within which large protected populations of macro-invertebrates can hide, with some animals leaving this protection and becoming sea otter food. There is also a heavy squid spawning off Monterey and Pacific Grove, and sea otters readily capture the dying post-spawning squid (page 124 of proposal). There are also dense kelp canopies containing snails and kelp crabs that are important to the sea otter. Sea otter densities in California are not below that of Amchitka, if anything they are higher in California when comparing equal rocky bottom areas. What was not included in the FOS critique was that there have been die-offs recorded both in Alaska and California wherein the animals coming ashore dead exhibited the same symptoms, i.e., emaciation often accompanied by enteritis which has been uniformly considered as nutritional die-offs by mammalogists working with the sea otter. During these times, n (total population) exceeds k (carrying capacity).

During the early December 1975 Mammal Conference at UCSC, I asked Ancel Johnson and Jim Estes of the FWS and Karl Schneider and Calvin Lent of the Alaskan Department of Fish and Game about this apparent difference between California average sea otter densities (12-13 animals per square mile throughout our range as opposed to around 60 animals at Amchitka), and the above reasons were related, with the conclusion by all that there really was not an obvious difference, if any. To several questioned, the amount of fish in the diet at Alaska alone could probably account for the entire apparent difference. I informed Jud Vandevere of this conversation with the Alaskan researchers and related all the facts above to Vandevere in the presence of Dave Zeiner (DF&G) and Dr. Aryan Roest, Calif. Poly. State Univ. San Luis Obispo. The fact that this statement was made indicates a lack of understanding of the sea otter's behavior and dynamics in relation to its environment by the signators of the FOS critique.

b. *"that numerous crabs, snails, and other food items can be found within the range (e.g., Hopkins Marine Station)"*

In all predator-prey relationships, there is a point at which the law of diminishing returns becomes limiting and eventually a "balance", often fluctuating, develops between the predator and prey in relative population levels. In the case of the sea otter, within its established range after the migrant front moves through and has removed most of the readily available exposed food items, their food supply consists more of species with a smaller biomass but still includes a few urchins and abalones that occasionally leave their protective habitat. Other larger food items that continue to be important are the motile crabs and octopus or highly camouflaged animals such as decorator crabs and rock scallops. Studies by Lowry and Pearse (1973), North (1965), Minter (1971), Faro (1972) Limbaugh (1961), Ebert (1968a), and Wild and Ames (1974) all document very clearly that the patches of exposed edible invertebrates are virtually eliminated resulting in greatly reduced numbers of food items available to the colonizing otters that occupy the area after the migrant front moves on. The migrant front animals must continue to immigrate in order to sustain the large concentration of animals at the periphery of the range.

The word *"numerous"* was not quantified in any way in the FOS critique nor were any references given to substantiate the claim that there was an apparent surplus of food available throughout the year. This statement may have originated in the paper by Lowry and Pearse (1973) wherein a density of 0.22 urchins per meter square was measured in the Hopkins Marine Station area where, as stated by Lowry and Pearse: "although these animals have been preyed upon by sea otters for over 10 years, their densities are substantial..." All these urchins were tallied deep in crevices where otters could not reach them, and, as mentioned above, this area of the coast contains exceptionally rugged rocky bottom areas. It is no surprise that relatively large densities of crevice

populations exist there. Minter (1971) relates an almost complete removal of edible invertebrates on siltstone reefs devoid of crevices only a mile from Hopkins Marine Station. Edible exposed and crevice invertebrates are not *"numerous"* in this area of Monterey Peninsula (page 159 of proposal).

If scientific collectors wished to find *Tegula* or *Pugettia* in a kelp canopy or urchins and abalones in protected crevices in the sea otter's range, they would probably find enough to consider them "numerous", but to a sea otter finding enough of these food items to maintain its energy requirements of from 5,000 to 8,000 calories per day (page 178 of proposal), these scattered, motile, or hidden food items may not be "numerous", especially during winter storms. John Pearse and Tuck Hines (UCSC) have recently complained about the scarcity of *Pugettia* in the canopy areas of Hopkins Marine Station for use in the class study they are presently conducting. I related to them that there are *Pugettia* in Carmel Bay, some of them quite large, but that they were not in great numbers, certainly not "numerous". The FOS statement, not backed up with comparative density data between areas inside and outside the sea otter's established range, is unsubstantiated.

c. *"that the amount of time sea otters spend feeding within and outside of the established range is similar."*

What is important here is the source of data used to come to this conclusion. If studies were conducted in California throughout the year and especially during stormy periods in late winter, then the statement might be true. However, I know of no such studies; certainly none have been published. Most of the food analysis studies were only for a short period, usually less than a month or two in the summer period, and were conducted in a scattered distribution, five in a migrant front area and five inside the established range (Table 6 of proposal).

What the FOS reviewers may be missing is that starvation is not chronic but acute, occurring almost entirely during the late winter storm periods. Lensink (1962) made this quite clear:

> "The fact that mortality is heaviest in those are groups most sensitive to food shortage, and that deaths can be correlated directly with storms of relatively short duration rather than the entire stormy period, lends support to the theory that stress resulted from acute food shortage rather than chronic starvation. Parasitism, malnutrition, tooth decay or other debilitating factors may contribute to the effect of food shortage."

Sandegren, Chu, and Vandevere (1973) also noted prolonged feeding periods through stormy periods but not during calm weather, and during the March 1974 census flight (Appendix F), most of the animals sighted were feeding throughout the mid-day flight period during which time the animals are usually resting. It is during and immediately after these conditions that the largest number of dead animals appear on the beaches (Morejohn, Ames, and Lewis, 1975).

The carrying capacity may not be exceeded during any time of the year except during these extreme storm periods, and throughout the remainder of the year, there may not be a significant difference in the time spent foraging between animals in the migrant front and those in the established range in comparable habitat. This condition has been repeatedly referred to in the literature (page 119 of proposal).

d. *"that the population is slowly expanding both north and south of the range all suggest that the population is not food limited at this time."*

This statement in the FOS critique is not consistent with many statements in the proposal and in most of the Alaskan sea otter literature that sea otters do not immigrate into new areas in large numbers until the food supply is reduced.

Page 89:

> "The sea otter is considered a residential inshore animal that in a stabilized population has a home range (Kenyon 1969). Lensink (1962) noted that sea otters are reluctant to immigrate between islands until population pressures stimulate such movements and that along newly occupied shoreline, 'sea otters generally have not tended to exploit unoccupied areas, even when adjacent regions offer suitable habitat and no barriers exist to retard movement.'"

Pages 94-95:

> "The formation of the migrant front and its continual movement peripherally to leave a previous home range is to obtain food. These groups remain in an area until food becomes scarce and then move on in groups as well as individually. Dick Burge (California Department of Fish and Game, pers. comm.) has conducted underwater surveys of invertebrate densities in the Point Estero area from 1967 through 1974 and noted a sharp decline in invertebrate food items in 1971 and 1972, the time when the migrant front moved southward to Cayucos Point. No exposed abalones and sea urchins were observed since 1971 in the transects (page 189) and the area could obviously no longer maintain such a large aggregate of sea otters. The colonizing animals are less dense in concentration and are probably controlled in numbers by the food items available, resulting in starvation, or if new foraging areas are available, by immigration."

Along the Pismo clam beaches of Monterey Bay (Miller, Hardwick, and Dahlstrom, 1975), sea otters did not move into adjacent beaches of dense clam populations until the clams were reduced to low levels by the sea otters, substantiating the predictable behavior that otters do not immigrate unless food becomes scarce.

Paragraphs 10, 11, and 12. *Population estimation and status.*

Our census technique has been thoroughly reviewed by one of the foremost biometrical statisticians, Dr. Lee Eberhart, University of Washington. His analysis of our census and conclusions will be made available to the Mammal Commission, hence, detailed rebuttal of the FOS critique is not necessary here. However, some comments are submitted.

Paragraph 11.

The simulated curve (Figure 2, page 23 of proposal) is a collation of population counts and estimates from 1938 to present superimposed on a curve developed from estimates of population based on the area occupied between migrant fronts throughout the 1938 to 1975 period. This figure is one of the more important and informative contributions to the knowledge of the sea otter's past and present status in California. The simulated curve from habitat occupied is

qualified by several assumptions, some of which are not thoroughly empirically tested, and these points on the curve are not intended to be used as factual estimates.

It has been well established that the relationship between the numbers of sea otters present and the carrying capacity of the habitat is a function of food availability, and when food becomes scarce, starving surplus animals either immigrate into new foraging areas or die. It is not an assumption but a fact that immigration of large numbers of sea otters does not take place until food becomes reduced at which time the migrant front moves on (see above section). The migrant front moves only into the next, nearest food-rich area and does not remain scattered over a wide section of the newly foraged area either singly or in small aggregates. A few wanderers and advanced foragers do separate from the migrant front at times but these animals do not represent the limit of the range (Section III A 3 of proposal).

The ground counts in 1938 were conducted by trained field researchers, and the other counts and estimates included were also considered valid. There was a series of aerial counts from 1965 through 1972 that were not included in the text of the proposal (they appeared in Appendix C, Wild and Ames, 1974). These counts were not standardized and were not adjusted by empirical ground truth observations. The flights between 1965 and 1967 were conducted from a twin Beachcraft during which little or no circling was attempted, and the data are considered useful only for distribution of the animals observed. The flights from 1968 through 1972 were more systematic, but were not adjusted by ground truth observations. The counts referred to in the FOS critique (paragraph 12): *"The actual aerial counts between 1969 and 1975, on which the curve <u>should be based</u>"* cannot be used as representing total population estimates without corrective ground truth factors. For instance, aerial counts in 1968 and 1970

varied from 377 to 1,040 animals throughout the total range. Which of these counts represents the actual population? Suppose the May 1970 flight in which 1,040 animals tallied would have been cancelled; the population then in 1970 would have been considered to be only 612 animals (September 1970 count). Or, if only the October flight was scheduled for 1971, the population count, according to the criteria set by the FOS critique, would have been 715 animals. In January 1972, 1,060 animals were counted. Surely the population could not have increased at this rate over a 3-month period.

A series of data collected by Judson Vandevere in conjunction with the State's aerial counts in 1969 and 1970 (Appendix F, Table 1 and Figure 1) lends credence to the use of subjective criteria to aerial counts to arrive at rough estimates. Kenyon (1969) also used these subjective adjustments for scouting conditions. In 1969-1970, there were seven flights in which complete aerial and shore counts were available for a given segment of the coast. There was a correlation coefficient of $r = .98$ that the aerial counts varied in the same degree as the ground counts, and there was a correlation coefficient of $r = .92$ that the subjective censusing ranking was representative of both the aerial and ground counts. The total range counts from the air varied from 438 to 1,014 animals in the year's period. Thus, it is obvious that it is statistically unsound to consider the request in the FOS critique that the *"curve should be based"* on *"actual aerial counts"*.

In 1973 only one flight was made, in 1974, two flights, and in 1975, one flight. All these flights were made in conjunction with ground truth observations, and all were in the December to June period when censusing conditions are more likely to be optimum. None of the counts on these flights were as high as the maximum counts in the 1965 to 1972 period. Several censusing procedures have been incorporated into the recent flights that were not practiced

prior to 1974. These are conducting the flight over a 3-day period instead of 1 or 2 days, not counting the entire area from the plane (ground counts are now used exclusively near certain residential areas), and a much larger area is now covered, including extensive sandy beach areas. Different weather conditions over a 3-day flight can result in variable counting success. For instance, in the June 1975 flight, observing conditions were optimum on the first 2 days with observer success at 76 and 77 percent. On the third day, a strong wind created poor conditions with only 51 percent of the animals observed in the ground truth stations reported by the aerial observers. Had the same conditions prevailed on the third day as existed on the first two days, the number of animals counted from the plane would have been over 1,220, almost 200 more than ever counted previously be aerial observers. As it was, by counting all the animals observed in the ground truth stations not tallied from the air, plus the aerial counts, over 1,300 animals were counted on this flight, with an estimate of over 1,700 animals.

In summary, the simulated curve was not intended to replace our accurate estimates. This curve was constructed to relate probable population numbers and increase over the past 60 years.

Paragraph 11. *"To be even more outrageous, this arbitrary figure of 12 was increased to 13 ('to be more realistic')... This assumption is not justified..."*

The 12 animals per square mile (actually 12.8 in Wild and Ames, 1974), and 12.5 as estimated in the June 1974 flight) was not arbitrarily determined. The figure was computed by dividing the number of estimated animals by the square miles of habitat out to 120 feet depth, regardless of the bottom type. For the computations of the simulated curve, these figures were rounded off to 12 animals per square mile, primarily because in the earlier years, a greater percentage of the population was distributed in richer areas. The slightly higher

figure was considered more realistic than rounding off to 12 animals, as was done in the conservative approach used in relating present population densities in the text of the proposal. The slope of the simulated curve is the important parameter, and whether the entire curve is 20 animals less throughout does not change its representation as a possible population growth curve.

Paragraph 12. *"It appears, rather, that the true population curve, based on these figures, would be almost flat..."*

As noted above, no true population curve can be construed from counts alone, even shore counts. The plotting of counts from shore and plane (Appendix F, Table and Figure 1) would not better represent a true population curve during the 1969-70 period than the use of the counts from flight from 1969 to 1975 as suggested in the FOS critique. Wild and Ames (1974, Table 1) did adjust their counts by subjective censusing condition ratings as did Kenyon (1969). They disclosed an increase throughout the 1968 to 1972 period from about 1,250 animals in 1968 to over 1,650 animals in 1972. The ranks of excellent, good, fair, and poor as applied to the aerial counts in Appendix F resulted in a correlation coefficient of $r = .92$ between aerial and ground counts, giving more credence to these adjustments than at first assumed. In summary, our sea otter census is probably one of the more accurate and reliable censuses now being conducted on any wild population of mammals, and that estimates of population numbers indicate an increasing population over the past 10 years.

Paragraphs 13-17: adult to pup ratios.

This is the one error noted in the proposal, and it has been corrected. The adult to clinging pup ratios for 2 days of the March flight were transposed, and the corrections in Appendix F have been made. The error did not in any way create erroneous conclusions. In fact, more animals were added to the estimates, further substantiating the census technique. The only other comment necessary

here is that, contrary to Vandevere's data, ground truth observations made in December 1973 and March and June 1974 and in June 1975 did demonstrate more clinging pups in June than in March. Our data are from the entire range; Vandevere's data were only from certain areas. Tom Loughlin's (UCLA) data for 1976 indicate more clinging pups present in the Monterey Peninsula in March than in February indicating the peak of abundance of pups is probably between March and June, not in February.

Paragraphs 18-23: *"Taxonomic treatment of the sea otter."*

Enclosed is a draft copy of Aryan Roest's reply to the Davis and Lidicker paper. The latter paper is not considered a valid synonymy, and the State recognizes one subspecies existing from the Commander Islands to California. No new information was included in the Davis and Lidicker paper and we repeat our opinion on page 82 of the proposal:

> "Until new material is available and analysis proves otherwise, Roest's synonymy is valid."

It is unfortunate that the Davis and Lidicker paper was not submitted after the present study by Lidicker on electrophoresis of blood proteins was completed. It is also unfortunate that the paper was not reviewed by mammalogists who had conducted research on sea otter, particularly in California, so that statements about differences in behavior between California and Alaskan animals could be judged. The paper by Davis and Lidicker and the subsequent comments by Roest have been included in Appendix J of the proposal. A few minor comments are in order.

Paragraph 21. There was no *"attempt...to establish gene flow between the southern population and populations to the north."*

The proposal pointed out there may be a potential for this in the future, not that there is presently gene flow.

Page 80:

> "If the small groups in Washington and Oregon become established (possibly they now are, but only hindsight will tell), a condition will exist that was not present in the pristine population, i.e., a gene pool of the Aleutian Island stock off Oregon directly intermixing with wanderers from the California stock, or visa versa. Few conclusions can be made from this new potential mixing of genetic variants along the coast at this time, but determination of subspecies based upon genetic isolation would be at best tentative."

Paragraph 22. *"...only 13 individuals were noted in the 1975 count (pers. comm., Ron Jameson, Dec. 6, 1975)."*

Jameson was contacted subsequent to this communication and stated that his count of 13 sea otters was not a complete census of the entire area these animals might frequent, but only at the locality where they have usually been observed. Jameson did not infer that this count was a total census, and he doesn't know how many animals there now are in Oregon.

The July 1975 count by Jameson may have actually revealed a sudden decline in the Oregon population inasmuch as a California Department sea otter count on May 17, 1976 revealed only four sea otters in the area from Crescent City to Coos Bay. Three animals were on Blanco Reef, where most of the population remained over the past two years, and one at Orford Reef. Weather conditions were optimum for scouting and if many animals were missed, they would have had to have been hauled out, which is unlikely.

Paragraphs 24-25: *The marine environment and the sea otter.*

The FOS comments pertaining to the impact of pollutants on sea otters are biased by statements of opinions, subjective interpretations, misleading extrapolations, and unsupported value judgments. Some of the questions which were raised are valid, but most of the interpretations were not.

The information presented in the FOS critique adds nothing to alter our conclusion that at the present level, pollution has not and is not now threatening the California sea otter population. There are two papers not reviewed by

the Department before submission of the proposal. These are the Phillips (1975) and Gilmartin et al. (1976) papers. These studies, and several more that came to the surface in our review of pollution, have not added any information that would necessitate change in our policy and the conclusion that pollution is not a threat to the otter. We recognize the potential threat of these substances if their occurrence increased significantly in the otters' range. Thus the Department will continue to monitor the levels of these contaminants in the tissues of sea otters and in other levels of the marine ecosystem within their range.

Paragraph 24. The statement made in the proposal on page 22:

> *"The habitat within the proposed mainland range has not been adversely affected by man's activities, and no adverse change in the structure of ecosystems is evident due to pollution or other man-caused influences."*

This statement was written in the context of sea otter habitat and husbandry of the animal, and upon seeing this statement taken out of context, it is apparent that certain interpretations of the words could make the statement untrue in some conditions. Obviously, the filling-in or dredging-out of an area for a harbor, placing of a piling for a pier, mooring of a boat, or a small amount of oil from a motor boat all could have some influence on some ecosystem, but the changes may be minor and not affect the sea otter in any way. The statement in question is true as it stands when referring to empirical evidence of adverse effects to sea otters of any change in the habitat due to man's activities.

This topic could have been carried to another interpretation in the proposal: that the activities of contemporary man have enhanced and are enhancing the environment for the otter, but there is not enough solid information to include these statements in the proposal. For instance, contemporary man does not hunt the otter as did the aboriginals; therefore, some stress has been removed. Flood control measures on some rivers in central California have apparently decreased siltation entering Monterey Bay to the point where there is a reported negative

sand deposition along some of our beaches. Compared to 20 years ago, the habi along Cannery Row is not much improved due to the removal of turbid effluent f the canneries. Lush kelp canopies appeared in 1960 soon after closure of most of the canneries and following the warm water years of 1957-1959. This return of giant kelp occurred 3 years before arrival of the sea otter. The building of rock breakwaters off Monterey, Moss Landing, Santa Cruz, and Morro Bay over sandy bottom probably created habitat for more sea otter food items and kelp growth. The pilings of piers and floats in harbors provide substrate for mussels and barnacles that are eaten by otters. Crabs also tend to concentrate under piers and are in turn eaten by sea otters.

Man has also removed natural carnivorous predators along the beach areas that most likely preyed upon hauled-out otters, and, unfortunately, the bald eagle no longer preys upon sea otters in California due to man's occupation of coastal bald eagle habitat. Bald eagles cannot tolerate man's activities near their nesting sites.

There have been no deaths to sea otters and no evidence exists indicating a reduction in reproductive potential due to any form of pollution. With the nearly complete preclusion in the use of DDT, this pollutant will not become a problem in the future. Sewage loads will be non-existent in the future between Seaside and Morro Bay, except for the trickle at Point Sur (Table 1 of proposal The receiving waters of the proposed cosmopolitan southern Monterey Bay outfall will be off the Salinas River, away from any concentration of sea otters. The Santa Cruz outfall will likewise be relocated farther offshore and at the same time eliminating the Aptos and Pleasure Point outfalls. All these proposed activities will most likely create better conditions for the sea otters; however, there is no evidence at hand to prove existing conditions are actually harmful to the otter. For clarification, this section has been rewritten to prevent any misinterpretation of our intent.

Paragraph 25.

This paragraph makes a value judgment as to "high" levels of environmental contaminates. With regards to chlorinated hydrocrabons, the residue levels are not "high" or "relatively high" when compared with levels of residues in other marine or terrestrial mammals which occupy a similar level in the food chain. The statement also overlooks the fact the residue levels in the body are in many instances a dynamic situation where residues are both gained and lost. In such an instance, residue levels may not increase indefinitely with duration of exposure. The study by DeLong, et al., 1973, did not prove chlorinated hydrocarbons to be the cause of reproductive failure in sea lions. The study showed a difference between levels of DDE residues in prematurely born sea lion pups and full-term pups. The study does provide justification for further study of the situation; however, it is not strong supporting evidence for the concerns expressed in this paragraph by Friends of the Sea Otter.

Paragraph 26.

Again, this is an unsupported value judgment. Levels of pesticides and PCB's are not "high" compared to those found in other healthy marine and terrestrial mammals. There is no evidence to indicate that "more vigilance" would turn up reproductive effects in the sea otters caused by pesticides or PCB's. Current monitoring of the population would show any reproductive failures if they had or were occurring.

Paragraph 28. *"An analysis of sediments from 49 stations in Monterey Bay during 1970 and 1973 revealed <u>increasing levels</u> of the pesticide DDT and two of its degradation products, DDD and DDE (Phillips, et al., 1975)."*

This case of misleading extrapolation "increasing levels" is not justified based on only two sets of samples. Furthermore, workers familiar with sampling and analysis procedures for chlorinated hydrocarbon residues should be aware of

the problems associated with producing reproducible results in the parts per billion range near the low end of sensitivity of extraction methods and instrument accuracy. When dealing with such minute quantities, two aliquots of the same sample often vary 100 percent or more, say from 1 ppb to 2 or 3 ppb of resid Thus, representing increase by percentages are not meaningful when dealing with low residue levels of pesticides.

In Phillips et al., there are data on an increase in DDT but not DDD and DDE. The values in Phillips et al. in mean concentration ppb are:

	DDT	DDD	DDE
1970-71	3.1	2.3	5.4
1973	15.5	2.3	5.4

What was omitted from the FOS critique was that the use of DDT has been reduced tremendously since 1972 and that these values in Phillips's paper represent past concentrations of DDT in sediments of the Salinas Valley being carried into Monterey Bay by the Salinas River runoff. Even though there appears to be an increase of about five times as much DDT in 1973 as in the 1970-71 period, this increase did not occur throughout the Bay; in fact, there was a marked increase in the sediments at only 5 of the 19 stations (Figure 1, attached). These exceptionally large, isolated concentrations were mostly in the deeper portions of the Bay. The fact that there was no increase in average concentration of DDD and DDE further compounds the problem of evaluating potential effects of these compounds on marine life.

There are no data in the Phillips report that indicate the increase of "134%" will continue for 10 years as postulated in the FOS critique. There certainly will be a decrease of DDT in the silt loads in the future, and this pollutant will be sharply decreased in the future throughout the sea otter's range.

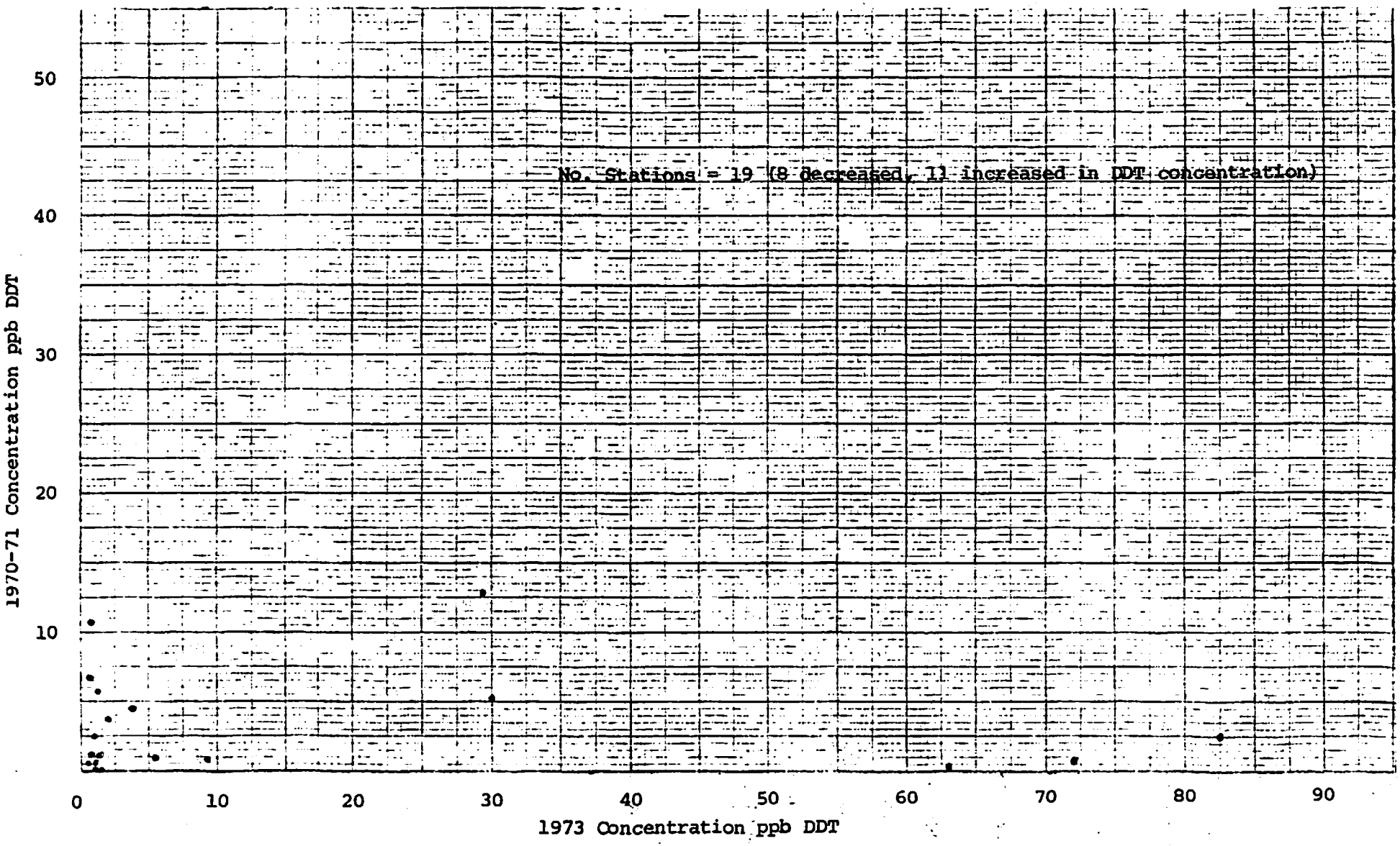

Figure 1. Concentrations of ppb DDT at 19 stations in 1970-71 and 1973. Data from Phillips et al., 1975.

Paragraph 29.

The data present are factually correct; however, interpretation of values such as 0.3 to 13 ppm of DDE or 0.5 to 4.0 ppm (parts per million) in sea otter liver tissue as "high" is an unsupported value judgment. This is not to say that such values are not of concern, but they do not indicate an imminent danger to the population.

Paragraph 30.

The statement about uptake of chlorinated hydrocarbons at the air/seawater interface is opinion and speculation. This was not proved by Rote in his thesis.

Paragraph 31.

The statement that the existing contamination of the marine environment in the sea otter range is "significant" as far as the welfare of the sea otter is concerned is an opinion. This opinion is not supported by current state of scientific knowledge pertaining to this subject. The last sentence is not proved by DeLong (1973).

Paragraph 32. *"A relationship found between manganese levels and the ratio of selenium to mercury in aborted sea lion pups indicated the critical need for future investigation of selenium and a possible corresponding relationship in sea otter pups. One otter pup, apparently born prematurely was chemically very similar to aborted sea lion pups indicating possible reproductive effects."*

(CONTINUED ON NEXT PAGE)

The juxtaposition of these two sentences, one describing a selenium/mercury ratio as being potentially injurious and the other pointing out the similarity of the chemical make-up of sea lion pups and a sea otter pup, leads one to believe sea otter reproduction may be affected by pollutants, particularly the selenium/mercury ratio. In reality, the chemical similarity of the two animals was in concentrations of calcium and potassium; selenium determination was not made of the sea otter pup. There is no evidence of sea otter reproduction being affected by pollutants. The highest number of pups-to-adults in California has been noted near Monterey, the only area within the range of a major sewage outfall as well as the location of the only major boat harbor in the present sea otter's range.

Sea lions have received considerable attention by researchers disclosing effects of pesticides and diseases on marine mammals. Hubbard (1968) described laboratory care of pinnipeds, and subsequent to the first known occurrence of *Leptospirosis* (Vedros et al., 1970) in sea lions, results of several more studies on sea lion diseases and pollutant residues were published. These were: the revelation of the presence of vesicular exanthema of swine virus (VES) (Smith et al., 1973); a report on relationship of premature births with high organochlorine residues (DeLong et al., 1973); bioaccumulation of heavy metals in some marine organisms including sea lion and sea otter (Martin, 1974); a survey of the diseases of free-living sea lions (Sweeny et al., 1974); a study on the probable causes of premature parturition in the California sea lion (Gilmartin et al., 1976); Martin et al. (1976) related mercury-selenium-bromine imbalance in premature parturient sea lions; and Rote (1975) related PCB data on Monterey Bay. In contrast, little research has been conducted on diseases of free-living sea otters because of the State and Federal restrictions to take animals since the early 1900's. Necropsy data collected since 1968 have revealed there are no

virus or bacterial pathogens, and field observations reveal a continuing high rate of reproduction. Sea otter pups that are found on beaches or "abandoned" usually appear during times of inclement weather. Many were emaciated, showing symptoms of under- or malnutrition.

Because of the lack of a detailed free-living sea otter disease study, there has been a tendency to equate or relate occurrences of sea lion deaths and symptoms to possible comparable effects upon sea otters; such as in paragraphs 25, 29, 31, and 32 of the FOS critique. This is not a proper comparison inasmuch as sea lion pups are markedly different from sea otter pups in many ways. Sea lions are piscivorous whereas sea otters are invertebrate feeders in California. This is manifested in the occurrence of relatively larger residues of DDT (incl. DDD and DDE), mercury, and selenium in sea lion tissues than in the sea otter and in relatively larger concentrations of cadmium (derived from the gut of invertebrates such as the abalone) and silver in the sea otter. Another difference is the complete geographic separation of sea lion and sea otter females in California. Mature female sea lions remain in southern and Baja California waters whereas sea otters with pups are found only north of Point Buchon in entirely different water regimes with varying differences in levels of the various pollutants. Other basic differences are that sea lion births occur only during a short period of the year on land whereas sea otter pups are born throughout the year in water. The social structure of crowded "harems" in the sea lion rookery is compared to the isolated birth and long-term care of the sea otter pup. The waters of southern California apparently contain higher levels of most of the pollutants, and VES and *Leptospirosis* occur in sea lions but not in sea otters. The combination of virus and bacterial infestation with certain disbalances of chemicals (mercury-selenium-biomine) and/or pollutants (DDT and PCB) may result in immunosupression resulting in

lowered resistance to diseases. Gilmartin et al. (1976) state:

> "If immunosupression occurs in sea lions due to high organochlorine tissue concentrations, response of the immune system to an invading pathogen would be tempered. DDT and PCB compounds could therefore induce an abnormal susceptibility to disease producing micro-organisms, including those causing premature parturition."

This problem does not occur in the sea otter because there are no enzootic diseases involved.

In spite of the presence of pathogenic diseases and higher concentrations of pollutants in the southern California bight, none of the authors above have noted a decrease in sea lion population numbers, and, in fact, recent censuses indicate a continuing if not increasing abundance of pinnipeds in California. The highest concentrations of cadmium along the coast have been measured south of the large urban areas of southern California (Martin, 1974), with lowest levels recorded in plankton in the sea otter's range and to the north. With the removal of most of the sewage outfalls in the proposed sea otter's range, pollution will not increase and under present conditions and trends will not become a serious problem to the otter. PCB introduction into the air and water is still a potential problem, especially around harbor areas, but indications are that this pollutant too will soon be reduced in the marine environment. Young et al. (1975) summarize the status of PCB concentrations in the southern California bight:

> "Rates of polychlorinated biphenyl (PCB) transport via several routes to the coastal waters off southern California have been quantified. Submarine discharge of municipal wastewater was the single largest source, contributing 5,400 kg of these synthetic organics in 1974. However, inputs via this route appear to be decreasing, as the corresponding estimate for 1971 exceeded 19,000 kg. One result of this continuing discharge is that bottom sediments around the largest outfalls contain up to 10 ppm PCB. Aerial fallout also appears to be an important source; the estimated deposition rate of 1254 PCB onto the coastal waters during 1973-74 via storm and dry-wather flow. Direct industrial discharges to San Pedro and San Diego Harbors did not appear to be a major PCB source, totaling less than 250 kg/yr. Although antifouling paints may have been an important source in the past, present inputs are negligible. Despite high levels measured in three major harbors, we found no evidence of significant PCB transport from these harbors to the adjacent coastal waters."

Paragraph 44. *"considering what has happened to the 96 Amchitka otters transplanted to Oregon which have been reduced in numbers to around 13 to 20."*

These transplanted animals cannot in any way be compared with a remnant residential concentration of sea otters that may be left isolated by a major oil spill. The animals transported to Oregon were poorly handled, and only about 30 finally settled down into one section of the coastline, the others apparently wandering about, not surviving. The remnant that finally settled down have been reproducing, with at least 15 pups being born in the area.

The subtidal food resources in the area of an oil spill would be greatly harmed only in rare conditions, mostly by highly volatile substances. The intertidal zone may be hard hit, and if the spill was residual or fuel oil, the bottom may become affected but only in spotty areas (page 136 of proposal).

Paragraphs 46-58. *Man vs. otter impact on resources.*

Not much can be added to what is in the proposal on this subject. There appears to be a lack of understanding on the reviewers' parts of a description of the status of fishery stocks and how much man and the sea otter can each affect each resource. Other reviewers may lack this understanding, and for this reason we have requested a review of this section by fishery researchers of the National Marine Fisheries Service of the Department of Commerce.

A statement in paragraph 48 of the FOS critique illustrates the reviewers' belief that our shellfish resources cannot be maintained by management techniques:

> *"It is difficult to believe, with the evidence in the document at hand, that the fisheries involved in the present controversy will not continue to dwindle to the point where costs of exploitation or recreation will exceed benefits or rewards."*

The Department has been managing the state's natural resources for over 50 years, and most of the shellfish fisheries have now leveled off at a sustainable, but often fluctuating, level. The Dungeness crab fishery north of San

Francisco has returned on schedule, as predicted last year, with a yield of over 12 million pounds (12 times that of last year). The San Francisco stocks have not returned as was hoped, but this problem is not because of over exploitation by fishermen.

Pismo clam stocks are in good shape except possibly for the beach north of Pismo pier. Because of good year class strength in the 1960's, the catches are increasing, not declining at most beaches throughout the range of the Pismo clam (except at beaches that have been foraged by sea otter where there is essentially no fishery).

Red and rock crab fishing has not declined at piers where otters have not foraged, and the sea urchin fishery is still in its development stage. The recreational abalone fishery is in good shape along the north coast; new laws are expected to head off a potential problem there. New proposed laws changing the commercial fishery to a limited entry status will solve the over-exploitation problem in southern California. All these conditions were mentioned in the proposal, but they are offered here to point out that these resources can be managed and will be viable fisheries, unless sea otters are allowed to occupy the entire California coastline.

Paragraph 47: *"emphasis should be given to what man is doing <u>now</u>, and will be doing in the <u>future</u>, to the otter's food resources..."*

Within the sea otter's range, man exerts no competition to the sea otter for its food items: no commercial fishery can exist and the recreational take of abalones in the intertidal crevices is of such minor importance that no conceivable competition can be exerted. We cannot consider any food item not in the sea otter's established range between the migrant fronts as "belonging" to the sea otter any more than we can consider all the vegetation, including the fields of wheat in the Midwest plains on the former range of the buffalo "belonging" to

the remaining buffalo, or the crops in the Central Valley of California "belonging" to the tule elk that used to be there but are now contained elsewhere.

Paragraphs 47 and 48. In paragraph 47, the State is asked to give emphasis on *"what man is doing now"*, but in paragraph 48 we are criticized for doing just that:

> *"It is certainly no news to the Department that shellfish stocks are declining both within and without the otter's range, and we see no point in belaboring the issue using the Department's own statistics."*

Paragraph 52. *"that the sea otters pose no immediate threat to the major Pismo clam fishing grounds from Pismo Beach southward..."*

This statement in the FOS critique completely ignores the prediction that sea otters may arrive at Pismo Beach within 2 or 3 years (page 216 of proposal). This must be considered an immediate threat, especially considering the movement of 107 animals to the south into the Pecho Rock area in January 1976.

Page 49. There are comments here, again, about shellfish being otter food when the otters have not as yet arrived. These food items do not "belong" to the otter until the animals occupy the area. The Department has been protecting and will continue to protect sea otter habitat from pollution just as we have been protecting all resources from adverse environmental influences whether or not they are being utilized directly by man.

Paragraph 52. There is another statement here that the sea otter poses *"no immediate threat"* to the Pismo clam stocks. This has been covered above, but the statement about the impact of skindivers on clam stocks needs attention. There were no figures presented in the proposal showing a *"steady and growing use of such 'virgin stock' areas..."* in either John Fitch's letter (Appendix C-2) or in Marine Technical Report 23 (Miller, Geibel, and Houk, 1974). Only about 2,000 clams (an insignificant number) were taken by skindivers off Pismo

Beach in 1972, almost no clams were taken in Monterey Bay in that year by skindivers, and off Zuma Beach, except at the very northern end of the beach, Pismo clams can be taken only skindivers because the intertidal and shallow subtidal zones are naturally not inhabited by Pismo clams. A skindiving Pismo clam fishery has been going on at Zuma Beach for many years, and there is no evidence that this area is being depleted.

It is heartening to read in the FOS critique that recreational users are *"enjoying the challenge involved"*. Those of us who have worked closely with recreational resources fully realize the value of active outdoor activity and the enjoyment that is derived from "facing the elements of nature" to get a few clams, abalones, etc. Recreation activity such as this is valuable for the good exercise it affords for the individual and for the family social interaction that occurs during these outings. Natural resources are valuable for these consumptive values as well as for aesthetic, commercial, or "food for sea otter" values, especially for those who wish to temporarily escape from urban stress.

Paragraph 53. The computation of the 3,000,000 clams *"reduced"* by clammers on one weekend in the FOS critique is totally incorrect. The use of 10 clams per person when the average of around 3 is realistic, the cause of death to 10 sublegals when less than 1 is more reasonable, and the figure of 150,000 clammers which probably has not been reached again (it was during a 4-day low tide cycle in a holiday period) invalidates the calculation. Note that in Table 16 of the proposal only 220,000 Pismo clamming days were estimated for the entire state for the year. This is a conservative figure; the real effort may be closer to 300,000 clamming days. To offer such a figure of 3 million clams *"reduced"* on one beach over one weekend as *"perhaps not too far-fetched!"* is unreasonable. Even if this could happen, there is still a viable population of clams and a

good sustainable yield to clammers from the Pismo Beach area, and no matter what the take was during the *"150,000 weekend"*, it has had no influence upon the present day fishery. Pismo clam stocks are in good shape (Miller, Hardwick, and Dahlstrom, 1975), and they will remain so as long as some unforeseen pollution problems do not manifest themselves.

Paragraph 54. There was not an increase of 250 percent in skindiving days and a 540 percent increase in number of skindivers from 1960 to 1972 as stated in the FOS critique. The actual figures (page 209 of proposal) are 152 percent and 436 percent, respectively, for increase in skindiving days and numbers of individual skindivers. There was an increase of 67.4 percent of abalone diving hours between 1960 and 1972.

Paragraph 56. *"There is little attempt made, from compelling evidence available, to suggest the otter's probable positive role in the ecosystem..."*

All the references to positive effects of sea otter foraging have been included in the proposal, especially those studies conducted in Alaska wherein Dayton (1975) and Estes and Palmisano (1974) noted increases in algal growth. Other than the increase in algal growth (understory algae) noted by the above authors and by McLean (1972) and Ebert (1968b) in California, there has been no *"compelling evidence"* of any other positive role of the sea otter. Theoretically, if primary productivity is enhanced so should the energy flow upward through the food webs, but so far this has not been documented. These concepts are too speculative and theoretical to be bases for any management regime. Our proposed baseline studies are designed to measure and document potential changes in community structure and energy flow through trophic levels. The papers by Lowry and Pearse (1973) and Estes and Palmisano (1974) upon careful review relate little empirical data on these subject; most of the information given was highly speculative and theoretical. These papers were important in that they point out possibilities

that should be explored, but careless speculation can stretch the scientific value of these studies beyond the intent of the authors. They do not show *"compelling evidence"* of increases in any organism other than algae. These papers were not reviewed in detail in the proposal, because of the almost total theoretical aspects of the information presented or, in the case of the Lowry and Pearse paper, the weak and questionable data that were used. For instance, in the Lowry and Pearse paper, the only reference to enhancement of *Macrocystis* growth due to sea otter foraging in the Monterey Peninsula area is of North (1965), a paper that was based on erroneous data and came to misleading conclusions (pages 160-165 of proposal). It was stated in the proposal that (page 6): "...enhancement of the large canopy forming giant and bull kelps has not as yet been documented; although in theory this could happen to some degree." Urchin removal by hand or by liming in southern California has resulted in denser giant kelp canopies, as well as other algal growth, but until a baseline study is conducted completely documenting the change in algal growth due to removal of urchins by sea otters, it is not scientifically proper to state this as fact. As pointed out in the proposal (pages 157-169), organisms other than giant kelp or other algae can occupy areas cleared of urchins or abalones.

Another highly speculative, and in fact theoretically unsound, statement in the Lowry and Pearse paper should be pointed out: "the apparent increase in numbers of *S. purpuratus* and abalones may be due to a recovery of these populations following the initial 'invasion' be sea otter, or, more likely, less thorough counting by North (1965), especially of animals in crevices." From Table 3 of Lowry and Pearse, only the North (1965) tally could be used although Lowry and Pearse do indicate correctly that North's counts were not made by removal of urchins from the deep crevices for counting. The Andrews 1934-35 and McLean 1960 counts were about 30 times as dense as measured by Lowry and

Pearse because they were made before the otter's arrival and demonstrate a dramatic decline. There were no comparative data collected in the Hopkins Marine Station area previous to the Lowry and Pearse study to justify the above statement that there was a "recovery" of *S. purpuratus*. The counts by Ebert (1968b) and Faro (1971) were not made in the Hopkins Marine Station area and should not have been included in Table 3 of Lowry and Pearse.

Another important aspect of the Lowry and Pearse study area is that the Hopkins Marine Station study area is probably one of the most productive areas for macro-invertebrates along the California coast. The rocky formations are of granodiorite and contain an abnormally high number of crevices compared to siltstone and other formations. The 0.22 urchins per meter square is probably the highest of any density inside the sea otter's range, and is entirely of urchins in crevices out of the reach of sea otters. It does not seem plausible that there could be a "return" in densities of urchins in the crevices where sea otters cannot reach "following the initial 'invasion'..." if otters could not have initially depleted these crevice populations. This is especially so since Lowry and Pearse have speculated that "abalones may directly or indirectly outcompete sea urchins for space. Such competition may account for the absence of sea urchins in the larger crevices at Hopkins Marine Station..." The high degree of speculation and lack of studies on all these interactions does not justify the FOS critique claim that there is *"compelling evidence"* of the otter's positive role, especially in California.

Paragraph 58. *"Throughout the report, data and information about northern and southern sea otters are mixed without explanation..."*

First of all, we do not recognize that there are "northern" and "southern" otters in terms of taxa. Basic physiological functions and behavioral traits are so far indistinguishable between animals in Alaska and California, and in

fact, the more the sea otter is being studied, the few reported (Vandevere, 1970) differences in behavior are being disproven (pages 82-88 of proposal). In spite of the apparent almost total similarity of sea otter behavior and dynamics throughout the range from the Commander Islands to California, all information given in the proposal was thoroughly referenced to disclose the geographic locality of the observation or study. Again, in this paragraph of the FOS critique, there is not a single example to substantiate the accusation that in the proposal our data *"cannot be honestly handled in this way."*

Paragraph 59. *"We certainly do <u>not</u> yet have an adequate knowledge of the ecology and population dynamics of the otters and of the factors which bear upon their ability to reproduce themselves successfully."*

Far more is known about the sea otter in California than on most species that are adequately being managed either for full protection or for consumptive use. We have considerable information on the reproductive behavior and success of the animal, and have documented an increase of animals in California averaging about 5 percent per year for about 60 years. The basic life history has been revealed through studies in California starting in 1938 and continuing on a sporadic basis until 1967 when the State began its investigations. The fact that we can fairly well predict the behavior of the animals now, have finally understood the importance and dynamics of the migrant front, and the fact that pollutants have not been proved to adversely affect reproduction gives us sufficient knowledge to claim the subpopulation is no longer threatened and that we have a completely viable, healthy population in California. We have sufficient information from a management regime of non-containment; the additional population dynamics information now needed is to understand the formation of the migrant front and effects of removal of extralimital animals for possible containment management. These parameters can only be determined through an experimental management proposal such as has been submitted.

Paragraph 61. *"We are strongly opposed to a waiver of the moratorium."*

Unfortunately, no reasons were given in the FOS critique for this emphatic stand. It is not possible to conduct the research studies in the proposal without a waiver to take animals. We must be able to capture animals for tagging, translocation, and other scientific studies. The experimental management research must be accompanied with a waiver to take animals in order to be functional.

Paragraph 64. *"the initial translocation and tagging program be specifically limited to five years,"*

The study will not be completed until the animals arrive at Miramontes Point, which may take as much as 8 to 10 years. It is axiomatic that our research procedures and findings will be monitored not only annually but constantly by the FWS as this is a joint study with an employee of the FWS already stationed at Monterey for this purpose. It would be remiss of us to wait until after a 5-year period to look for and disclose *"hazards"* to the otter population. This will be done on a continuous basis. Also, the baseline studies must be designed for a longer period than 5 years, and movements of animals into the control area off Pillar Point should be controlled until sufficient data are gathered. In particular, should a large group of animals by chance occupy the control baseline outside the proposed experimental containment range before the front arrives at Miramontes Point, they should be removed back into the area near the migrant front.

Paragraph 65. We are planning to conduct baselines, but it must be emphasized here that a true "baseline" which includes fixed transects cannot be established along an unconsolidated sandy beach where the substrate continually changes. The Department is currently conducting randomized sampling of the clammers and is making intertidal and shallow subtidal digs along these beaches. We do not anticipate a before-and-after sea otter foraging in Pismo clam areas now that

all the Pismo clam beaches have been occupied by the otter in Monterey Bay.

Paragraph 66. Again, there can be no baseline set up at Pismo Beach, and our present studies are sufficient to determine the impact of clamming on the beaches. It is difficult if not impossible to set aside an area where there is no impact by man. We can choose an area that is not normally used, but we will certainly not have this site officially set aside as a study area by the Fish and Game Commission. This would merely advertise to some people that there must be something important there to protect and would probably attract people to the area to take what they may think is theirs. We have been in the job of doing this kind of work for many years and know quite well how to conduct these experiments.

Paragraph 69. This type of study is outside the normal functions of the Department. This information would be important, but the intangible values of shellfish users should also be entered into this study. Nowhere in the proposal has an attempt been made to evaluate the "enjoyment or happiness value" of a recreational day, or the aesthetic value and thrill of a photographer taking pictures of the tremendously photogenic large exposed urchins and abalone, especially of the exposed patches in juxtaposition to other aesthetic forms involved in the interaction of species. These values will be lost in the sea otter's range, but we did not attempt to put a value to these. If such a study is done, by some other agency or private group, all these aesthetic values should be incorporated.

Paragraph 70. *"any Department efforts to 'educate' citizens about the proposed plan be carefully designed and tempered to inform the public..."*

For society to make up its mind objectively on this subject, the "bad" along with the "good" must be revealed. Department representatives have the legal obligation under State law and as scientists under scientific ethics to

relate the truth of a matter to the public. When a request for information is submitted, we must answer the question, and a judgment as to the reaction of the public toward the otter cannot be made by the informant. We have constantly attempted to play down any antagonism to the animal by the public in our dialogue, and we will continue to do so. There has not been a single example of Department informers stimulating illegal action of "hatred" toward the animal in the press. If such activities come to light, please inform Director Fullerton.

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Further Comments on the Taxonomic Status of the Sea Otter, Enhydra lutris

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Davis and Lidicker (Proc. Cal. Acad. Sci., 40 (14): 429-437, October, 1975) have recently taken issue with my paper on sea otter subspeciation (Roest, Contr. Sci. No. 252, Nat. Hist. Mus., Los Ang. Co., December, 1973). They present a re-evaluation of the published data and, contrary to my 1973 opinion, feel it shows that the California population of sea otters should be considered a distinct subspecies. Although superficially their re-analysis appears sound, it is actually deficient in a number of ways. Comments on some of their major points are presented below.

Specimens from between Prince William Sound and California were not included in the 1973 study, and Davis and Lidicker state that the nature of the variation which occurred in this area is therefore unknown. They suggest that variation in skull characters may not have been gradually changing in this region, as suggested in 1973. On the other hand, variation may just as easily have occurred as suggested, and evidence is available to support this view. Scheffer and Wilke (J. Wash. Acad. Sci., 40: 269-272, 1950) did examine the few specimens known from this region, and concluded that variation does exist. They reached a conclusion similar to mine -- that only one form of sea otter exists along the North American coast.

Davis and Lidicker state that recognition of a southern subspecies of sea otter more accurately reflects the "existing natural situation". The existing situation however, is not natural, but is rather an artifact of

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human activities. The existing 1700 mile gap in sea otter distribution, between Prince William Sound and California, is the result of extermination brought about by hunters engaged in the fur trade during the nineteenth century. Such an artifical break in the distribution of a species is not a valid reason for considering the populations surviving at either end to be subspecifically distinct. If such were the case, it would be possible to create subspecies at will, by exterminating the central populations in the range of any widely distributed species, which is of course absurd.

In their analysis of my published data summaries, Davis and Lidicker make use of Student's t and Chi-square tests. It seems naive to place greater confidence in these simple tests than in the more sophisticated BMD07M discriminant analysis, an analysis especially designed to emphasize differences between groups. The results of discriminant analysis clearly show a gradient of skull types from the Aleutians through southern Alaska to California (graphically shown in Figures 3 and 4, Roest, 1973). All four populations clearly intergrade with each other, even with a 1700 mile gap in the data. Not only are Davis and Lidicker's statistics simplistic, but they are also incorrectly applied.

Davis and Lidicker suggest the presence of a step, or change in slope, of the cline which relates the Aleutian and Californian populations. They attempt to show its existence by a breakdown of the data from the southwestern Alaskan sample. Their analysis is faulty however, and also forces the data. They state that the great circle distance between Port Heiden and Prince William Sound is only 435 miles, but a glance at a map of the area shows it is actually 1000 miles or more, as the sea otter swims. The distance is biologically significant only when measured along the coast,

since sea otters cannot travel great circle routes. From this 1000 miles of coastline a sample of 34 specimens was analyzed, an obviously dispersed sample which may actually represent several populations. To properly study any possible shift in a cline would require at least as many specimens from two or three locations, and an analysis more effective than Chi-square. Until such an analysis is made, systematists should be conservative in wringing information from samples representing such a wide geographic range.

The size difference between Alaskan and Californian otters may actually be greater than the data shown, according to Davis and Lidicker, due to the fact that total lengths for Alaskan otters are curvilinear, while those of Californian animals are standard measurements. However, the difference is more likely to be less than the data show, as is clearly indicated in the 1973 paper. Curvilinear lengths tend to be greater than standard lengths, so Alaskan otters are actually smaller than the data suggest.

Davis and Lidicker point out that California otters can be distinguished from Aleutian ones with more than 90% accuracy, citing an earlier paper (Roest, Proc. 8th Ann. Conf. Biol. Sonar and Div. Mamm., Menlo Park, 133-135, 1971). This is true only when discriminant analysis is used on the four variables which were finally selected from a total of 24 which were measured. Individually, none of these four variables is 90% effective in discriminating between the two populations. It is interesting to note that Davis and Lidicker accept discriminant analysis as used in the 1971 paper, but question the analysis by applying simpler tests to the 1973 data.

In contrast, E. l. gracilis, the sea otter of the Kurile Islands, is immediately distinguishable from all other sea otters, with 100% accuracy, using any one of four different skull characters, and in addition has a

differently colored underfur. Most workers would agree that differences between subspecies should be roughly equivalent. If the Kurile Island form is a good subspecies, as it appears to be, then the California population, which differs less, is less than a subspecies. To ignore gracilis, as Davis and Lidicker do, is to ignore an important part of the total taxonomy of the species.

"An obvious conclusion ... is that statistically significant differences can be found between the vast majority of population pairs. This serves to emphasize what is really intuitively obvious, namely that the ability to prove that two populations are statistically different in one or several characters is only a measure of the persistence and patience of the systematis To base formal subspecific descriptions on this kind of evidence seems to me to be almost meaningless ... " (Lidicker, Syst. Zool., 11: 160-171, 1962). I agree completely!

What is more important than the differences between Californian and Alaskan sea otters is the great similarity between them. What is remarkable is how little difference there is between these populations spread over such a great geographic range. A highly sophisticated analysis was necessary to emphasize the differences in four skull features, while 20 other features were not significantly different.

Apparent differences in behavior are also mentioned by Davis and Lidicker Although differences have been reported in the past, each succeeding year of observation tends to increase the similarities in the behavior of Alaskan and Californian otters (see, for example, Calkins and Lent, J. Mamm., 56: 528-529, 1975, and recent reports of the California Department of Fish and Game). Whatever differences actually exist are not surprising in such an adaptable

species, occurring over a wide range in different local environments. For example, near Sitka, Alaska, a transplanted population of sea otters from the Aleutians has been seen using stones to obtain large barnacles which are a source of food in their new location. Aleutian otters have only rarely been observed using tools, but in this different environment they have readily assumed the habit. Further, both Aleutian and Californian otters use tools in captivity, a fact already noted in the literature.

Davis and Lidicker refer to local differences due to the fact that the California population of sea otters has developed from a very small group, with limited genetic variability, which survived from the nineteenth century. This is the "Founder Effect", also mentioned in the 1973 paper. They ignore the fact that the development of unique features as a result of an originally small gene pool is not unique to the Californian population, however. Kenyon (N. Am. Fauna Series No. 68, 1969) mentions that relict groups of sea otters survived (in North America) in the Rat Islands, the Andreanof Islands, the Unimak Island-Sanak Island-Sandman Reef area, the Shumagin Islands, and the Kodiak Island-Prince William Sound area, as well as in California. Each of these populations was reduced to a relatively small group of individuals within historic times. Each of these populations had a potential for developing genetic differences, and may well have done so. Should these small differences, the result of less than 150 years of man-caused isolation, be emphasized in efforts to describe the natural (= original) taxonomic relationships in this species? I think not.

In addition to minor population variations, consideration should be given to the great individual variation found in sea otters. Individual variations are readily observed in any series of skulls, as has been recently discussed (Roest, Variation in sea otters, paper presented at Conf. on Biology and

Conservation of Marine Mammals, U. C. Santa Cruz, Dec. 1975). Wide variability is apparently characteristic of many larger carnivores, and has been noted in such species as Canis lupus, Ursus arctos, and Taxidea taxus, to name only a few. Rodents and other small mammals appear to be less individually variable, and hence small differences in populations are regularly recognized taxonomically, as in Thomomys and Dipodomys. Sea otters are not rodents, however.

I have briefly examined the main arguments presented by Davis and Lidicker in the above comments, and find them deficient. They do not understand the situation in sea otters or my analysis of it. In many cases their reasoning is preceded by an "if this is the case" or similar phrase, but the "ifs" are usually not appropriate, or could just as easily be interpreted the other way. In conclusion, I feel no valid reasons exist to change my original opinion regarding the taxonomic status of sea otters in California, as expressed in my 1973 paper. Although this population is not subspecifically distinct, it is of course a unique southern group, the only one occurring naturally in the contiguous United States. The continued survival of this isolated population is of considerable interest and import but should not depend on its taxonomic status, as Davis and Lidicker sugges